5. Pierced Microliths from the Deccan Plateau
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Source: *Man*, Vol. 62 (Jan., 1962), pp. 10-12
Published by: Royal Anthropological Institute of Great Britain and Ireland
Stable URL: http://www.jstor.org/stable/2795822
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OBITUARY

Kathleen Rishbeth: 1888–1961

3 Kathleen was born in 1888 and was barely two months old when her father, A. C. Haddon, Professor of Zoology in Dublin, started off for Torres Straits to study coral reefs. He went out a marine biologist: he came back to organize and lead the Cambridge Anthropological Expedition to Torres Straits, which had so profound an influence on the history of anthropology. And although A. C. H. came to Cambridge in 1893 to become the 'Father of Anthropology' there he retained his zoological professorship until 1901. Kathleen was also first and foremost a zoologist. She obtained a First in the Science Tripos in 1910, was later tutor to Newnham students, and the first University Woman Demonstrator. But she too felt the lure of anthropology, especially when accompanying her father on his travels to America, Australia and elsewhere. Their most adventurous trip was up the Fly River in 1914, exploring where no white woman had ever been seen before and living mainly on tinned salmon.

While Kathleen was still a child her father was busily collecting string figures and tricks. 'You can travel anywhere with a smile and a piece of string' was his motto. So Kathleen was an adept from her early years, which led to the publication of her *Cats' Cradles from Many Lands* (1911), *Artists in String* (1930), the popular *String Games for Beginners* (1934) and her appearance on television. She was Librarian of the Haddon Library at the Museum of Archæology and Ethnology in Cambridge, a member of the Council of the Royal Anthropological Institute and for many years a secretary to Section H at the meetings of the British Association.

Her marriage to Oswald Rishbeth (later Professor of Geography at Southampton) was a happy one, blessed with three children. But later came heavy calamities, her husband's prolonged illness and death, her son's attack of polio and finally her own illness which she knew to be fatal. All these she bore with astonishing courage and serenity. 'It's just one of those things one has to face,' she said. And with courage and serenity she died on 6 September, 1961.

A. HINGSTON QUIGGIN

SHORTER NOTES

Conference on Human Population Genetics in Israel. *By Dr. D. F. Roberts, Department of Anatomy, University of Oxford*

4 The Jewish peoples have been for so long, and so recently, the subject of oppression in the name of mistaken race theories that it was most appropriate that the first international conference to be devoted entirely to human population genetics should have been held in Israel. From 2 to 5 September, 1961, Professor J. B. S. Haldane, F.R.S., presided over the gathering of 120 participants at the Hebrew University, Givat Ram, Jerusalem; among them the heavy representation (74) from Israel itself demonstrates the interest and activity in this field there. Indeed the set piece of the conference, the magnificent demonstration of work in progress, left visitors with no doubt that the Jewish peoples are fast taking a leading place among the most thoroughly investigated groups (from the genetical point of view) in the world. Yet it also demonstrated the magnitude of the problems awaiting investigation, and the opportunities that Israel with its environmental and population contrasts offers for their solution. Thus the Jewish communities are generally similar in their haptoglobin and fingerprint patterns; yet among the more frequent genetic characters, colour-blindness, *Rh* and *ABO* blood-groups, thalassæmia and glucose-6-phosphate dehydrogenase deficiency show marked variations in frequency, and so too do the rarer genes, *e.g.* those governing familial Mediterranean fever, Tay-Sachs disease, pentosuria. Other sections of the demonstration concerned biochemical disorders and rare defects, the effects of environmental *versus* ethnic factors in 'Western' diseases with some genetic basis, and the genetic effects of mating systems in all the variety in which they are present in Israeli groups.

There were five formal conference sessions devoted to: migration and drift in polymorphic systems, adaptive values of erythrocyte defects, the influence of environmental changes and of ethnic factors on the incidence of some common diseases and malformations, genetic studies in isolates, and the concentration of rare genes in the past and in modern times. In each the level of the discussion both of the prepared papers and of the contributions from the floor was of consistently high quality, and their publication is awaited with interest.

The participants left with keen appreciation of the hospitality of their hosts, and of the efforts exerted to make the Conference the success that it was, notably by Drs. Goldschmidt, Sheba and Falk, but also by the many others behind the scenes. The visitors also carried away a more sympathetic awareness of the difficulties that Israel is facing.

Pierced Microliths from the Deccan Plateau. *By Dr. D. D. Kosambi, Poona. With three text figures*

5 The artifacts from the Deccan Plateau considered here and shown in the figures to the scale of 5 cm. originate in the late pre-pottery stone age (mesolithic). They are surface finds discovered by individual examination of several thousand artifacts gathered over a continuous many-branched track which extends north and north-west for 60 miles or so from Poona, and some 140 miles to the south-east. Seventy-three of the 74 drilled microliths were found in the middle stretch of this track, from Poona to about 78 miles south-east. The proportion of those pierced to the total number collected is about five per thousand. The heaviest surface deposits and the finest technique of knapping are characteristic precisely of the central portion of the microlith track.

The holes must have been bored with a firmly held bow drill. Bone or wooden drill bits would suffice, if fine sand were used as the abrasive. The rough stone points sometimes used by modern Indian stone-workers in a bow drill would be too brittle for the smallest perforations, which can be cleaned only with a fine needle. The largest hole (fig. 1, top picture, middle piece; site Kumbharvalan) is about 3·5 millimetres in diameter. The white thread shown in the photographs is Johnson's dental silk floss; where this could not be worked through, 36-gauge BSW copper wire was utilized. The prehistoric makers would have used hair

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or sinew. No microlith has been counted as pierced unless it could actually be threaded. Many were drilled too close to an edge, which broke off to leave an indentation; these have not been included even when the mark of the drill is unmistakable. None of these microliths are specially prepared as ornaments; those not shaped as tools of standard type are cores.

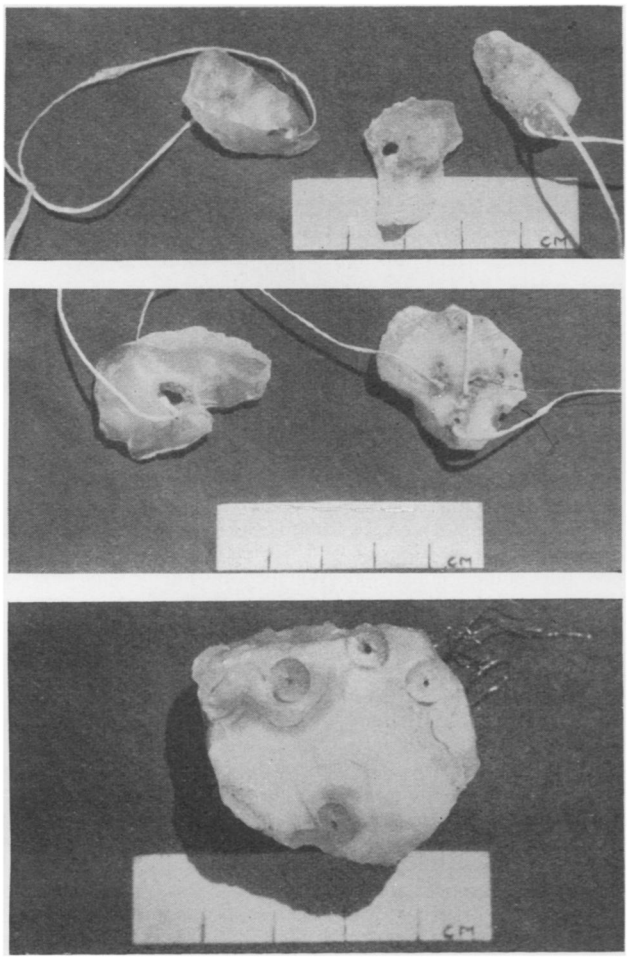

FIG. 1. PIERCED MICROLITHS FROM THE DECCAN
See text for sites of origin. Photographed with a 5-cm. scale

The chalcedony from which most of these artifacts are made is found locally imbedded in the Deccan trap. When it is translucent, a neat cylindrical discoloration some 4 mm. in diameter can often be seen about the hole. This might be due to the heat generated in drilling. Full advantage was taken by the microlith people to save labour by drilling through softer impurities, natural flaws, pits or partial holes in the material. Many holes start from both sides to meet at an angle in the middle, and this tricky procedure is responsible for some of the failures. Fig. 1, middle picture, shows two pieces from Vidni: in one, some imbedded material like felspar was eventually driven out by the drill, to leave an irregular hole; the other piece was drilled no less than nine times, with four successes (whereof one is amenable only to copper wire). Fig. 1, bottom figure, shows four major attempts, all failures, on a lump of chalcedony found by Mr. D. S. Chavda beyond the Poona Leprosy Hospital. Here, the two holes that can be threaded are through the side, and utilize some deep natural pits on that surface. A hole more than 3 mm. deep was apparently difficult to make with the tools employed.

The persistent attempts show that it was imperative, for some unknown reason, to bore through the particular bit of stone. Much better tools scattered in the amplest quantity at the same sites prove that the purpose of drilling could not have been mere convenience of transport. There is nothing special about the drilled specimens as regards size, shape, technique, facets, or choice of material—at least to a modern eye. The most plausible explanation should be that these were charms of some potency, to be used as amulets, pendants, earrings, or components of a necklace (*cf.* fig. 3). Occasionally, as at Barhanpur, Theur (fig. 2,

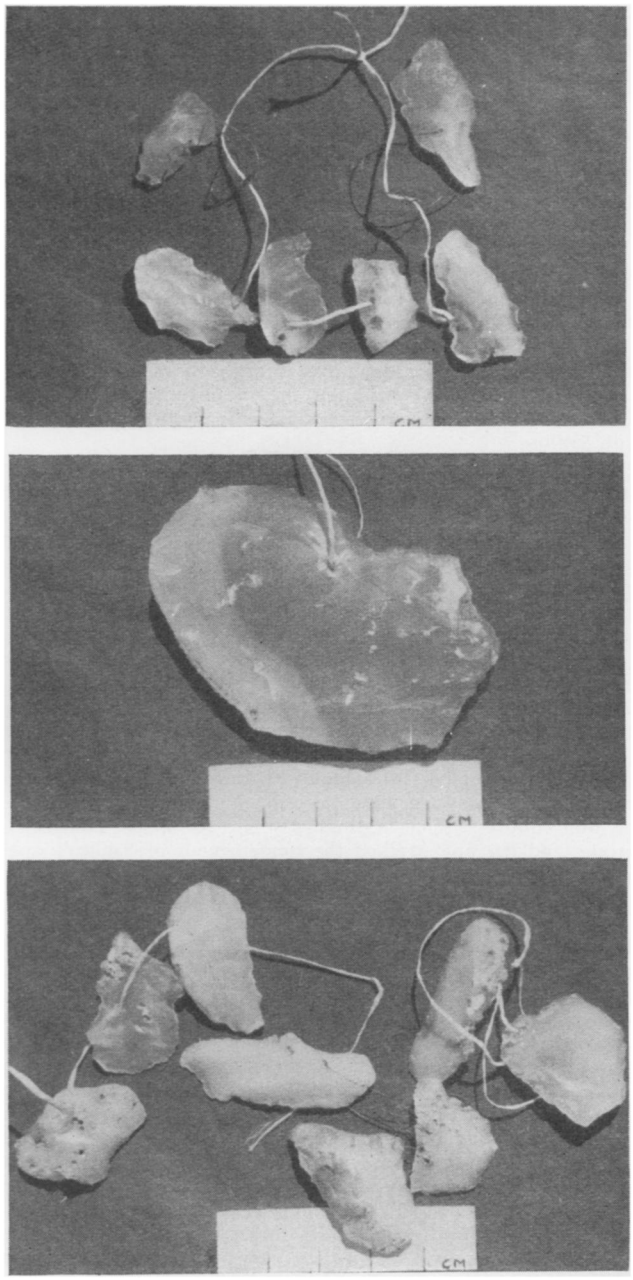

FIG. 2. PIERCED MICROLITHS FROM THE DECCAN
See text for sites of origin. Photographed with a 5-cm. scale

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top picture) and Bavdhan (fig. 2, middle picture), modern cults and prehistoric cult sites are found close to the microlith deposits. Yet one of the most prolific (fig. 2, bottom picture) loci is an

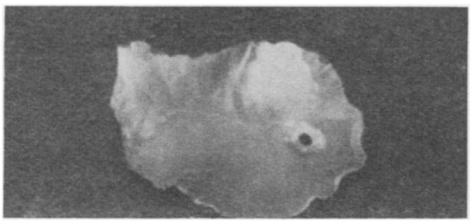

FIG. 3. A PIERCED MICROLITH FROM YERANDAVNE, POONA
Presumably a fish-shaped charm or amulet. Length 2·2 cm.

eroded field between Kurkumbh and Malad with no modern distinction of any sort.

Local peasants regard microliths as useless bits of stone, though some have heard that sparks can be struck from any flint by steel. Sheep-herders of the Dhangar caste still use freshly made sherds of chalcedony for gelding their rams. The wounds are free from the infection that would follow castration with unsterilized metal knives. (The same reason might have reinforced superstition in the Jewish use of flint knives for circumcision in the metal age.) These stone gelding knives, made on the spot and discarded immediately, are much larger and cruder than any mesolithic blades (which could only have been used mounted or hafted), and without the complicated facets and beautifully retouched edges of the prehistoric tools. The Dhangars make their 'knives' simply by hammering a lump of grainy chalcedony, placed upon a basalt anvil, with another heavy piece of basalt. The old artifacts (pierced or otherwise) are not recognized by the sheep-herders as tools, or even as products of human technology. The microlith track, however, may have left its mark upon the present. The great annual pilgrimage to Pandharpur, so beloved of the Maratha peasantry, probably originated in a seasonal booly over the Karha valley pasture lands through which the central portion of the track passes.

CORRESPONDENCE

Environment and Culture in Polynesia. *Cf.* MAN, 1961, 180

6 SIR,—I read with some surprise the review of M. D. Sahlins's *Social Stratification in Polynesia* by J. D. Freeman in the August issue of MAN. Curiously, *whatever* the merits of the substantive issues that he raises, his criticism is self-invalidating, even though the self-invalidation does not necessarily prove Sahlins right nor place us in entire concordance with Sahlins.

The substantive issues are fundamentally two, both involving a single case, Samoa, on which, he asserts, Sahlins's case stands or falls. Sahlins asserts that there is a form of kin organization in Samoa which he calls a 'descent line,' heretofore unrecognized. Freeman asserts that this is incorrect. Samoa, he asserts, had a ramage system quite equivalent to those in Hawaii, Tonga, *et al.* Therefore, one side of Sahlins's correlation is assertedly destroyed with respect to the case of Samoa—granting, for the sake of argument, that Freeman is correct. Elsewhere, Freeman cites Sahlins's assertion that Samoa possessed clustered, ecological resource areas, in contrast, say, to Hawaii. Freeman asserts that this is untrue and that the resource areas were distributed in a manner similar to those of Hawaii and Tonga. Freeman thereby assertedly destroys the other half of the correlation with respect to Sahlins's Samoan case. We shall show that on Freeman's own grounds, as well as in respect to a third type, the atoll cultures, which he singularly fails to discuss, Freeman is wrong, and has not attacked the fundamentals of Sahlins's case, but at most modified the interpretation of the relevant material to which the hypothesis refers.

Towards the latter part of the review, we read this odd sentence: 'If Sahlins's hypothesis were true (as it is not) [*sic*!] it would constitute an example of extreme environmental determinism, for it would mean that natural environments differing but little (*e.g.* northern Tonga and Samoa) could be shown to stand in a direct causal relationship with descent systems of two highly specialized kinds (*e.g.* a ramified system in Tonga and a descent-line system in Samoa).' First, one may ask how a hypothesis can be an example of anything in 'the real world.' Hypotheses are statements in a language; examples of determinism are events external to the language. Second, it is to be noted that Sahlins's argument asserts that the environments of Tonga and Samoa did not differ 'but little,' but were substantially different in significant aspects, especially with regard to the distribution of resource-producing areas. He would argue, I believe, that if they were 'but little' different the social organization of production and distribution should also be 'but little' different. This argument involves precisely the two correlated variables which Freeman has discussed separately above. Freeman has asserted that Samoan ecology is not different from Tongan, and then, later, asserts that the social organization is not essentially different from the ramage system of the Tongan type. In fact, what Freeman has done, assuming his data to be correct, is to give another example of Sahlins's correlation of the Hawaiian type. If the data are not correct, Sahlins's original correlation stands. Freeman's asserted disproof of a correlation has merely negated both sides of the original correlation for Samoa in the same direction, thus maintaining the hypothesis of correlation. Had he been able to show that, indeed, Samoa had a descent-line system but a distribution of resource areas like that of Hawaii, or *vice versa*, then a restating of the hypothesis might have been necessary. Even a restating of the hypothesis, however, does not necessarily imply that its central assertions are incorrect, as Freeman surely knows. His claim that the entire case for the correlation of social structure with environmental types is destroyed by the disproof for the case for Samoa is, therefore, of course, not validated, and, in any case, does not disprove the truth of the *hypothesis*.

In passing, the important point should be noted that Freeman has basically misunderstood Sahlins with regard to the relationship between environment and culture. In his effort to shy away from apparently any sort of determinism between the two, Freeman raises the bogey of ultra-mechanical environmental determinism when he says that Sahlins's hypothesis 'would constitute . . . extreme environmental determinism . . .' This it would not since the whole relationship of the societies to their environments entails the partially independent variable of the technology. The latter, for example, in Polynesia, involves the presence of a fairly elaborate horticultural practice rather than, say, a purely hunting technology which would give different kinds of correlations if, for lack of prey on some atolls, it would, in fact, support human life at all. It should be noted, too, that Sahlins is fairly careful, on the whole, to cling to the conception of correlation rather than to that of cause, which Freeman, in the quotation cited in the previous paragraph, suggests that Sahlins holds.

Though I tend to agree with Freeman that there is an element of circularity in the definition of levels, I feel that Freeman's strictures about Sahlins's use or non-use of statistics are specious. Statistics, as Freeman surely knows, are only meaningful in terms of some priorly analysed categories, however impressionistic such analysis may be. One gathers statistics within these categories with the intent of confirming previously analysed situations with greater exactitude or deciding doubtful cases or establishing norms from which one can measure departures. In the absence of data for purely statistical quantification, Sahlins has operated only with the first tools of discrimination—those of qualitative analysis. In view of the fact that many of the societies which he deals with no longer exist,

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